LETTERS TO THE EDITOR

Regarding "Screening for abdominal aortic aneurysm: A consensus statement"

With interest I read the consensus statement formulated by Kent et al on screening for abdominal aortic aneurysms (J Vasc Surg 2004;39:267-9). Among other issues, it is stated that when evaluating the cost and effectiveness of screening programs, the accuracy of the screening test and prevalence of the disease must be considered. Randomized controlled trials (RCTs) are considered the gold standard for evaluating the effectiveness of interventions and guiding decisions on patient management. The authors state that the benefit of screening programs has been demonstrated in 6 prospective randomised studies. However, to this date only 3 RCTs have been published,[1-3] while the final results of a fourth still have to be awaited.[4] In these 4 studies a 21% to 68% decrease in aneurysm-related deaths was observed. In 2 of the studies, this mortality reduction reached statistical significance.[2,3] One of these studies was biased because only in-hospital mortality was recorded.[2]

The prevalence of aortic dilatation >3 cm in the male general population is 4% to 8%. However, only a small portion of these men will eventually qualify for an operation because the diameter of the aorta has reached 5.5 cm and the risk of rupture exceeds 1% per year. Although the relative risk reduction (RRR) in aneurysm-related mortality seems remarkable, the absolute risk reduction (ARR) ranges between only .05% and .21% due to the low prevalence of men with large aneurysms. This implies that 476 to 2000 men need to be screened to prevent 1 aneurysm-related death (NNS).

When regarding these data, it is a question of who will benefit from a screening program for all men aged 60 to 85 years and women aged 60 to 85 years with cardiovascular risk factors, because for women it has been shown in a subgroup analysis that a screening program does not reduce aneurysm-related mortality at all due to the even lower prevalence of abdominal aortic aneurysms (Table). [6]

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Author	*Men's age*	*RRR*	*ARR*	*NNS*
Vardulaki[1]	65-80	21%	.20%	500
Lindholt[2]	65-73	68%	.21%	476
Ashton[3]	65-74	42%	.14%	714
Lawrence-Brown[4]	65-83	28%*	.05%*	2000*

*Data derived from Lederle FA, 2003.[5]

Reply

Dr Koelemay, in his letter to the Journal of Vascular Surgery, questions the findings of the recently published consensus statement advocating screening for abdominal aortic aneurysms (AAA). We recognize that screening for aneurysms has been a controversial topic; in fact, this controversy was the impetus for the organization of the panel that generated this document. Re-evaluation seemed warranted in light of the significant amount of new data that have become available over the past several years regarding screening for aneurysms.

When considering screening for any disease, multiple factors must be evaluated. Broadly, these include the invasiveness and cost of the screening test, the prevalence of disease, the associated mortality and morbidity if the disease is left undetected, and the risk and cost of treatment. It is difficult to isolate any 1 of these individual variables since each contributes to the overall cost and efficacy of screening. Koelemay's primary concern is with one of these variables, the prevalence of disease. He calculates that to prevent 1 aneurysm related death it may be necessary to screen over 500 patients. In terms of cost-effectiveness, however, a 5-minute inexpensive test applied to even 2000 patients may be worthwhile if one death can be prevented.[1] It is worth noting that a similar analysis revealed that 1200 to 2500 patients need to be screened with mammography to prevent 1 breast cancer–related death.[2,3]

Certainly, screening for aneurysms compares favorably with other well-accepted screening tests. We and others have conducted cost-effective analyses in which all of the important variables are considered. We found a cost-effectiveness ratio associated for screening of $11,215, a number that compares quite favorably with screening for breast cancer ($16,000) as well as coronary artery bypass for left main disease ($9,500).[1] Koelemay points out that it is difficult to show a dramatic effect for AAA screening on the overall death rate. This is because ruptured AAAs accounts for a relatively low proportion of the overall mortality of any population. Deaths from other causes, such as myocardial infarction and cancer, statistically overwhelm those from AAA (or for that matter prostate or breast cancer). However, randomized trials have repeatedly shown a dramatic effect of screening on death in patients who have aneurysms. Koelemay also raises concerns about the efficacy of screening for AAAs in women. We agree there are very few data that allow the definition of a subpopulation of women that should be screened. The available information does suggest that women with multiple comorbidities have a reasonably high prevalence of aneurysmal disease. Thus, we have recommended screening of this patient cohort, albeit this area deserves further evaluation.

In conclusion, we feel there are strong supportive data that favor screening for AAAs in selected populations. The incidence of ruptured AAAs in the United States has not changed over the past 20 years; up to 30,000 people die of ruptured AAAs each year.[4] We are hopeful that screening programs for aneurysms will have an

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impact on this number and reduce the mortality and morbidity associated with aneurysmal disease.

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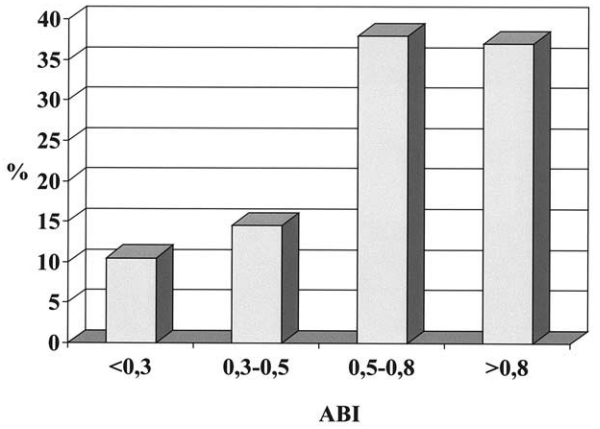
Regarding "Deep vein harvest: Predicting need for fasciotomy"

We read with interest the article presented by Modrall et al (J Vasc Surg 2004;39:387-94). The authors present a large experience, but we were surprised by the high incidence of fasciotomy. As a whole, 17.8% of the limbs required a fasciotomy after deep vein harvest. The incidence after operation for infected aortic prosthesis was 19.3%. This does certainly not reflect our own experience!

We have been using the technique of in situ reconstruction with the deep veins for prosthetic infection for more than 10 years and some of our results have been published previously.[1,2] Our overall experience with in situ reconstruction, using the deep veins, in infection after aortic prosthesis covers now 90 patients (aortoenteric fistula, n = 25; "primary" infection, n = 65). Five patients required a partial graft excision (ie, iliofemoral venous interposition), but the majority (94.5%) underwent complete graft excision and in situ replacement by venous aortofemoral (n = 75) or aortoiliac (n = 10) graft. With regard to our technique, we would like to emphasize that, with just one exception, we never harvested the deep veins in combination with the ipsilateral greater saphenous vein. After harvesting the veins, the wounds are closed immediately and the limbs are packed by an elastic bandage before continuing the operation. Intermittent pneumatic compression is used routinely for 5 days.

Overall, in this series of 90 patients, the deep veins were harvested in 172 limbs and deep vein harvest was complete—according to the definition of Modrall et al—in 165 limbs (96%). Preoperative ankle-brachial indexes (ABIs) were known in 160 limbs (93%). The mean ABI was 0.74 ± 0.28 and an ABI ≤0.5 was noted in 40 cases (25%) (Figure). As in the series of Modrall et al, the need for fasciotomy was left to the clinical judgment of the surgeon, with 4 patients (4.5%) requiring a fasciotomy within 30 days of the operation. This means a limb-related incidence of only 2.3%. One fasciotomy was performed during the initial operation on a patient who was operated on emergently because of acute ischemia. The other 3 patients developed an acute ischemia after unilateral thrombosis of the venous graft in the immediate postoperative period. Repair consisted of thrombectomy, and concomitant fasciotomy was felt necessary. The preoperative ABIs in these 4 limbs were 0.2, 0.4, 0.6, and 1.1, respectively.



Distribution of ankle-brachial index (160 limbs).

Because 3 of the 4 fasciotomies were the result of technical failures, we cannot confirm the data presented by Modrall et al. Knowing that 87% of the fasciotomies in their series were performed during the initial operation, we also wonder which parameters they have to justify this position and whether the authors have any data that a fasciotomy was in fact really necessary.

From our series, we instead conclude that acute venous morbidity with compartment syndrome after deep venous harvest represents an exceptional event. This is also in agreement with some other (smaller) series, in which acute venous hypertension and compartment syndrome were not mentioned as a problem after harvesting the deep veins.[3-6]

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Reply

The authors thank Dr Nevelsteen and colleagues for their insightful comments based on their experience with "deep vein"